

*SOME THOUGHTS ON THE S-R ISSUE AND
THE RELATION BETWEEN BEHAVIOR ANALYSIS AND
BEHAVIORAL NEUROSCIENCE*

JAY MOORE

UNIVERSITY OF WISCONSIN-MILWAUKEE

In a recent review of Donahoe and Palmer's *Learning and Complex Behavior* (1994), Shull (1995, pp. 353–354) questioned whether the emphasis of adaptive neural networks in their biobehavioral approach constitutes a return to an S-R psychology that is inconsistent with the behavior-analytic conception of operant behavior. Donahoe, Palmer, and Burgos believe that it does not constitute such a return, and seek to allay any concerns by clarifying certain aspects of the biobehavioral approach. I would like to examine two of the many highly interesting issues raised in their discussion. The first is fairly general: the relation between behavior analysis and behavioral neuroscience. The second follows from the first, but is more specific: the relations among the interpretation of behavior, the experimental analysis of behavior, and levels of analysis. I am not sure I can resolve any unresolved matters, but perhaps I can contribute to their discussion from a perspective that might enable someone else to do so.

The Relation Between Behavior Analysis and Behavioral Neuroscience

It seems to me that the relation between behavior analysis and behavioral neuroscience is often misunderstood; I suggest that they are complementary. To be sure, Skinner's *Behavior of Organisms* (1938) was a "declaration of independence from physiology" (Skinner, 1995, p. 157) in that it called for behavior to be regarded as a subject matter in its own right. Nevertheless, a knowledge of physiology is certainly not irrelevant to the study of behavior, and a thoroughgoing behavior analysis does not imply such irrelevance. Skinner's argument, in 1938 and subsequently, was that behavior is not to be

treated merely as evidence for inferences about entities from neural, mental, or conceptual dimensions, and that such entities are not to be given special causal status. The contribution of physiology will be to show how exposure to such factors as contingencies of reinforcement changes an organism, and how it is that the changed organism then behaves differently at a later date. The information from physiology will come from methods appropriate to it as an independent discipline, and not as inferences from the very same behavior that it is supposed to explain (e.g., Skinner, 1974, pp. 218–223).

Thus, it seems to me that a comprehensive science of behavior is appropriately concerned with two issues. The first is: How is an organism's behavior functionally related to its environment? The second is: How do the neurophysiological systems of the organism mediate those functional relations? The first issue is the province of behavior analysis. It contributes to a science of behavior by analyzing the control exerted by contingencies operating at the phylogenic, ontogenic, and cultural levels. The second issue is the province of behavioral neuroscience. As Skinner noted,

A behavioral analysis has two necessary but unfortunate gaps—the spatial gap between behavior and the variables of which it is a function and the temporal gap between the actions performed upon an organism and the often deferred changes in its behavior. These gaps can be filled only by neuroscience, and the sooner they are filled, the better. (Catania & Harnad, 1988, p. 470)

Given that behavior analysis and behavioral neuroscience are viewed as complementary, the basis for the complementarity still needs to be clarified. I suggest that it is pragmatic, rather than logical or reductive. That is, once we know how physiological inner states are functionally related to behavior, then predic-

Address correspondence and reprint requests to Jay Moore, Department of Psychology, University of Wisconsin-Milwaukee, Milwaukee, Wisconsin 53201.

tions about an organism's behavior (or even interventions aimed at control) may be based on information about the current status of those inner states, rather than on a possibly inadequate specification of the history responsible for the states (e.g., Skinner, 1953, p. 34; 1969, p. 283; 1974, p. 221). If we raise the question of whether psychology needs to consider underlying neural mechanisms to become adequate, we have departed from our pragmatic concerns and have made an excursion into an epistemological or reductionistic dimension, where one sort of knowledge is valid if and only if it can be grounded on another.

In any event, if we continue on this theme of the nature of the relation between behavior analysis and traditional biological science, we can also see that a parallel exists between the history of evolutionary theory and that of behavior analysis (e.g., Catania, 1987, p. 255; Donahoe, Burgos, & Palmer, 1993, pp. 18–19; Skinner in Catania & Harnad, 1988, p. 111). The history of evolutionary theory suggests that Darwin's original ideas about evolution and Mendel's statistical laws became fully influential only some 60 years after they were first presented, when biochemists and others provided a persuasive account of how genetic mechanisms and DNA mediated evolution through natural selection. These events are often referred to as the "modern synthesis." The parallel is that behavior analysis now awaits its own modern synthesis, in which a specification of the neural mechanisms underlying the processes by which reinforcement selects behavior will presumably provide the answers for behavior analysis that are analogous to the answers that the gene and DNA provided for Darwin's and Mendel's theories.

I raise the point about the parallel histories because, given Donahoe and Palmer's (1994) biobehavioral approach to complex behavior, it is important to ask whether adaptive neural networks are the best candidate for the neural mechanisms and for filling the "two gaps" inherent in a behavior-analytic account. Donahoe et al. argue that adaptive neural networks are precisely such a candidate. This stance raises the issue to which we may now turn.

The Relations Among the Interpretation of Behavior, the Experimental Analysis of Behavior, and Levels of Analysis

Donahoe (1993, p. 453) defines *interpretation* as the use of principles derived from experimental analyses and constrained by formal (i.e., logical or mathematical) considerations to provide an account of events that occur under conditions that preclude controlled experimental analysis. Donahoe further suggests that the greater part of the scientific enterprise is interpretation, and that the greater part of Skinner's writings are interpretations rather than experimental analyses (e.g., Skinner, 1957). In this regard, Donahoe et al. clearly view adaptive neural networks as appropriate and meaningful *interpretive* devices for filling the two gaps in a behavior-analytic account:

Understanding is achieved through scientific interpretations that are constrained by experimental analyses of behavior and neuroscience. The most compelling interpretations promise to be those that trace the cumulative effects of reinforcement through formal techniques, such as adaptive neural networks, as a supplement to purely verbal accounts. (p. 193)

I see two critical matters in this approach. The first concerns levels of analysis: Are appeals to adaptive neural networks interpretive in the traditional behavior-analytic sense? Behavior-analytic *interpretations* traditionally account for behavioral events in terms such as discriminative stimuli, responses, and reinforcers, but without identifying those elements through controlled, formal experimental analysis. Behavior-analytic *explanations* traditionally remain at the level of behavior rather than, say, at the level of physiology. However, if interpretations are taken as explanatory activities, should they also remain at the level of behavior rather than at the level of neurophysiology? Is anything important lost when interpretations are at the level of underlying neural mechanisms, even if they are formally constrained by reputable principles?

The second critical matter concerns confirmation. Interpretations ordinarily apply principles that have been confirmed elsewhere, in independent analyses. However, many of the principles of the adaptive neural networks are

not yet confirmed through experimental analysis of the relevant physiology. For example, is there existing, independent physiological evidence that accounts for variability in the topography of an operant, as well as moment-to-moment variability in its rate or in its emission in the presence or absence of a discriminative stimulus? To be sure, Donahoe et al. readily acknowledge that further information confirming any contribution of adaptive neural networks will ultimately have to come from neuroscience itself. Nevertheless, I confess that I do not know the answers to the thorny questions noted above, and I anxiously await their resolution.

Summary and Conclusions

In conclusion, despite behavior analysts' convictions about the value of behavior analysis, the rest of the scientific community appears to be unconvinced. One need only compare the number of behavior analysts that are currently on the faculty of the most highly ranked, prestigious universities, to, say, the number of cognitive psychologists. What can be done to increase the acceptance of behavior analysis?

One possibility is to develop better techniques of prediction and control. However, in so doing we may fall victim to the fallacy of the "better mousetrap." The Dvorak keyboard has a demonstrably better arrangement of keys for a typewriter or computer terminal than does the traditional QWERTY keyboard, but it has not yet become dominant. Behavior analysis offers a range of effective techniques for prediction and control that have been available for many years, but it has not become dominant either. Thus, the answer seems to lie beyond the merit of techniques that are relevant to prediction and control. If you build a better mousetrap, the world does not necessarily beat a path to your door.

Another possibility, and the one that is relevant to the present discussion, would be to specify the underlying neural mechanisms that mediate the selection of behavior through reinforcement. Perhaps we should regard the question of the acceptance of behavior analysis as a behavioral question rather than as a question about logical validity. Behavior analysis will be just as true and valid without a specification of the underlying neu-

ral mechanisms, just as the work of Mendel is just as true without a specification of genetic structure and DNA. If the question is a behavioral one, then perhaps we need to view the problem as a shaping problem, and start at the level of the subjects whose behavior we want to shape. If people are more willing to accept behavior analysis when the underlying neural mechanisms that fill the gaps are specified, then perhaps we should consider providing the specification. To be sure, knowledge of the underlying mechanisms would also yield pragmatic benefits. This knowledge would ultimately open new avenues for the control of behavior, as in therapeutic interventions, although we are admittedly still a long way from interventions at this level.

In any case, I know of no better candidate at present for the underlying neural mechanisms than adaptive neural networks. Thus, they seem to be worth pursuing. It seems to me that we can do so with the recognition that models are a concrete context within which to examine a given phenomenon and assess the grain of truth of some statements that relate to the phenomenon in question. We need not embrace instrumentalist concerns that neural models, any more than any other kind of theory, are propaedeutic to scientific knowledge.

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*THEORY AND BEHAVIOR ANALYSIS:
COMMENTARY ON DONAHOE, PALMER, AND BURGOS*

J. E. R. STADDON

DUKE UNIVERSITY

The target article raises a number of interesting issues and comes to several conclusions with which most can readily agree. Operant and Pavlovian conditioning are measured with different procedures but are not completely different processes; Skinner's goal of explanation at the level of moment-by-moment behavior is a desirable one; and neurophysiology does not invalidate behavioral laws. I can add only a couple of comments.

First, although Skinner often urged moment-by-moment analysis ("Farewell my lovely!" and so forth), his consistent antagonism to real theory inhibited theories at that level. Because only "laws" (like Weber's law) seemed to be acceptable in behavior analysis, theory has for years been stuck at the level of molar laws. This development was not, as Skinner complained, a reaction against his ideas, but was in fact the only path he left open. After all, if all theory that "appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1950, p. 193) is prohibited, but we want to explain things anyway, then molar laws are all that is left. Skinner

was not worried by the fact that his prescription would have ruled out most of the great theoretical developments in physics and biology, from the atomic theory and the theory of the circulation of the blood through genetics and the wave theory of light. Almost every important theoretical advance in science has postulated "events taking place somewhere else [or] at some other level of observation." Donahoe et al. are quite right to insist on the necessity for real-time theory, but are wrong to credit Skinner with sympathetic anticipation of their proposal. Far from promoting the solution, Skinner's stance on this issue was part of the problem.

My second comment concerns the main point of the target article: whether reinforcement acts to strengthen responding or stimulus-response connections. This seems to be a straightforward empirical issue: Is operant learning context dependent or not? In other words, after training does responding decrease when the context is changed, or not? Is there a generalization gradient? The answer obviously is, "Almost always." With very few exceptions, operant learning in mammals and birds is subject to stimulus generalization decrement. Therefore, reinforcement must act not just on the response but also on its connection with context. Nevertheless, not all organisms show context dependence. The

Correspondence concerning this article should be addressed to J.E.R. Staddon, Department of Psychology: Experimental, Duke University, Durham, North Carolina 27708 (E-mail: staddon@psych.duke.edu).